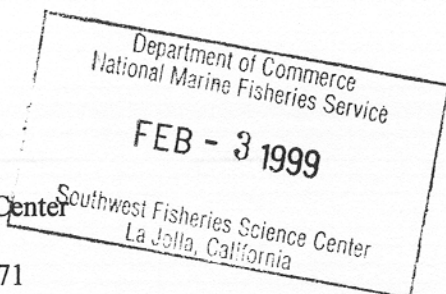


INTER-AMERICAN TROPICAL TUNA COMMISSION  
COMISION INTERAMERICANA DEL ATUN TROPICAL

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February 3, 1999  
Ref: 0051-812

Dr. Michael F. Tillman  
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Dear Dr. Tillman:

This is in response to your letters of January 15 and the attached documents resulting from the NMFS research concerning the International Dolphin Conservation Program. You noted that if our comments are to be taken into account in your initial finding you would need them by January 29, 1999. We were not able to respond by that time because that we had a week of meetings starting on January 25, and the Interim Scientific Committee meeting the week before that.

Members the Commission's staff attended the very constructive meeting on January 21, 1999, at which the preliminary estimates of 1998 dolphin abundance were discussed. I understand that the estimates will be revised after those discussions, and I may wish to comment on any such changes. In addition, we have examined data on sightings of offshore spotted dolphins by our observers in the first half of 1998. These show significant numbers of sightings outside the surveyed area. When the data are available for the second half of the year we will compare those with the surveyed area and advise you of the results.

I have the following comments regarding the paper of ETP dolphin habitat variability.

The second sentence of the concluding paragraph states that "Significant environmental effects on dolphin abundance are not evident in yearly MOPS results ..." There are very large inter-annual variations in the estimates of stock sizes from those surveys; some differ by 2 and 3 times. Given that variability, I find it hard to see the value of a conclusion that significant environmental effects on dolphin abundance are not evident.

The paper identifies a measure of habitat preference of dolphins referred to as habitat availability, and the author notes that this is related to "encounter rate of schools (an index of abundance), rather than a population response such as reproduction or survival." Then, after comparing the variability of this index over the period of the MOPS surveys and 1997-1998, he concludes that "*environmental variability cannot explain apparent lack of recovery of NE offshore spotted and eastern spinner dolphin stocks since 1986.*"

Apart from begging the question of whether there is an apparent lack of recovery, it is not credible to leap from an index that is not related to reproduction or survival to a conclusion about recovery of stocks.

Finally, I think that if you are to attempt to look at the environmental effects on dolphin populations you should take account of the fact that you are dealing with populations with many age groups. The dolphins present during 1986-1990 would have experienced oceanographic conditions for up to 20 years before then, and any analysis should consider how conditions over that time might have influenced survival and reproduction. Similarly, the dolphins present in 1997-1998 have experienced a different set of conditions leading up to those years. Even if you look at a shorter time series, such as the 1992-1998 period, the conditions prevailing in 1998 are not representative of what happened in the 7-year period in question that you want to analyze. This is especially clear when you consider that one of the largest El Niño events of the century took place just before your surveys. Survival and reproductive rates reflect not only the current environmental conditions, but the cumulative effect of several previous seasons that determine the condition of the animals, their growth rates, etc. Any conclusion about the environment and any recovery cannot be taken from the 1998 data set alone.

I have the following comments regarding the paper of stress in mammals.

The paper argues that it is plausible that stress resulting from chasing and capture affects the population levels of one or more dolphin stocks, and indeed concludes that it is plausible. This approach suffers from the difficulty of proving, rather than rejecting, a scientific hypothesis. I suspect that a different selection of literature, or a different reading of it, may support the conclusion that it is plausible that stress resulting from chase and capture does not affect the population level of any dolphin stock. The two alternatives are, of course, not contradictions, but merely demonstrate the difficulty of establishing a useful statement of that form.

The definitions of stress and stressors could be explained more fully. What is meant by a "state of threatened homeostasis"? Does this contradict the statement on p. 15 that "when the duration of a stressor is limited, the physiological effects of are beneficial and of no adverse consequences"? If the consequences were not adverse, does this mean that homeostasis was not threatened and that no stress had actually occurred? It should also be made clear that the stress-related mortalities that are of most concern are those that occur before or after capture; mortalities during capture are already observed and are at a level that should allow the population to recover.

Because of the lack of data on stress for dolphin populations affected by the EPO purse-seine tuna fishery, information from studies of terrestrial mammals and other cetacean species is being used to conclude that it is plausible that stress due to encirclement could have an effect on population growth. The following caveats should be included in the conclusions.

- 1) Marine mammals appear to have some differences in stress response from terrestrial mammals. According to the review, dolphins show different cortisol and aldosterone responses to stress, and have a different thyroid metabolism (St. Aubin et al., 1996) from those of terrestrial mammals.
- 2) It should be cautioned that the other cetacean studies typically involve coastal populations with little or no experience with capture. Dolphin populations in the EPO have had 40 years of experience with this fishery, with individuals likely having multiple experiences each year. The one exception is 29-year-old study on the Sarasota Bay bottlenose dolphin population whose individuals have been caught and handled regularly on the order of once per year. According to St. Aubin et al. (1996), these captures result in a "mild stress response." If one ignored the species and habitat differences (and this point argues that one shouldn't ignore them), one could argue that the EPO populations, with their even longer and more-frequent capture histories (about 8 times per year over a period of 40 years), would display an even milder stress responses.

3) It should be explained that the levels of stress involved in these other cetacean studies are greater than that experienced typically by dolphins in the EPO. Some of these studies involved capture and handling of the animals by humans, while other studies involved animals that died due to entanglement in nets. ETP dolphins are chased and encircled with a mile-long net, but are then released with minimal or no handling. When these other studies are cited, more detail should be presented about the extent of the stressors (i.e., the capture and handling procedures prior to sampling).

The conclusion that "Habitat utilization, foraging efficiency, and social activities are all likely to be disrupted" (p. 54) is probably not true. Radio tracking and food-habits studies conducted by the NMFS and by the IATTC (contracted by the NMFS) have indicated that spotted and spinner dolphins are largely nocturnal feeders. Given that the fishery does not operate at night, it is unlikely that feeding is being disrupted significantly. Food habits studies also indicate that yellowfin tuna feed during the day, making the hypothesis by Au and Pitman (1986) mentioned in the same paragraph that the dolphins gain nutritional advantages from the tuna unlikely.

The review also concludes that the separation of herds can also be stressful, and that kin-related groups are likely to be broken up. The paper by Scott and Cattanach (1998) and radio tracking studies indicate, however, that herd composition is fluid and fluctuates throughout the day and night. While there are likely subgroups that are more permanent, it is also likely that these subgroups remain together when they evade capture or when the herds split up during chase.

It is argued that "In investigation of hearts from human drowning victims, CBN has been considered to be the cause of sudden death (Lunt and Rose, 1987). Given the conclusions of Cowan and Walker (1979) that stress cardiomyopathy occurred in dolphins killed in tuna purse-seine nets, it seems possible that CBN could lead to the death of animals captured in the ETP fishery" (p. 57-58). Given that Cowan and Walker examined specimens from the fishery where the most common cause of dolphin mortality is asphyxiation, wouldn't it be more reasonable to assume that CBN is associated with entanglement in the net or being trapped under a canopy? Several of the studies cited (Young et al. 1997, Myrick and Perkins, 1994) have suggested that stress due to capture has occurred, but their results have been confounded by the fact that the dolphins most likely died due to entanglement in the nets and subsequent asphyxiation.

In regard to the argument that stress is disrupting reproduction so that the expected density-dependent responses by the exploited stocks were not observed, the review does not mention the most recent NMFS study (Chivers and DeMaster 1994) which did show the expected density-dependent responses (including the proportion pregnant). Chivers and Myrick (1993) found that the proportion of lactating mature female spotted dolphins had increased over time, which would be difficult to explain if stress was disrupting lactation. Given these data, the conclusions about the adverse impacts on reproduction of EPO dolphins (p. 51) should not be as strong as they are.

While the mention of the Myrick and Perkins paper (p. 40) is necessary for such a review, you should include the extensive criticisms of the paper (see Scott background paper in Curry and Edwards, 1998 and the letter from Edwards to Tillman that summarized the five outside reviewers' comments).

The section on capture myopathy fails to cite Cowan and Walker's (1979) study, which looked specifically for evidence of capture myopathy and failed to find it. The review states, however, that the Cowan and Walker study found that "several of the dolphins apparently died of massive cardiac reaction to stress and were documented to have cardiac lesions consistent with those produced in laboratory animals injected with catecholamine and humans ... thought to have died of stress cardiomyopathy." The original authors stated this more cautiously. They presumed that a few dolphins died of "stress" because

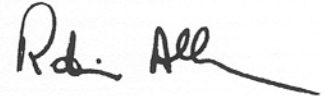


of the lack of signs of any other cause of death. They presented evidence of "contraction banding" in the heart of some dolphins (not identified as the same ones above), which is described as a reaction to fatal situations, as one might expect if the dolphins died due to entanglement.

The paper could have discussed the effects of the selective pressures of the fishery on the populations of dolphins. One could argue that "stress-susceptible" individuals have already been selected out of the population after a 40-year fishing period during which thousands of sets on dolphins have been made per year.

Once again, I am sorry these comments come later than you had wished, but nevertheless hope you take them into account.

Yours sincerely,

A handwritten signature in black ink, appearing to read "Rd. Allen", with a long horizontal flourish extending to the right.

Robin Allen  
Assistant Director

CC: Commissioners